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Biogeosciences Discussions

Interactive comment on "Availability of phosphate for phytoplankton and bacteria and of labile organic carbon for bacteria at different pCO_2 levels in a mesocosm study" *by* T. Tanaka et al.

Anonymous Referee #3

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This paper is one of at least five to be written (one is now published) about a large mesocosm experiment to examine the effects of elevated pCO2 levels on biogeochemical processes of coastal marine systems. This paper is focused on P with some data and discussion on glucose uptake and DOC. The overall message of the paper is that elevated pCO2 does not affect significantly (but see below) P or glucose dynamics.

If we accept that there are no significant differences due to pCO2 levels, then this paper has a huge problem in trying to extract something positive out of negative results (the lack an effect). I really sympathize with the authors (all that work and nothing to publish??), but I have to point out that this paper is full of data and lots of words

(and several equations) but without much new to say. The lack of an effect perhaps is worthwhile pointing out in a short note, but not such a long paper. Perhaps there are other ways of organizing the data that would result in more interesting observations.

It is perhaps interesting to compare the glucose uptake with the results of Riebesell et al. (2007 Nature). That paper reported that increasing pCO2 results in higher production of transparent extracellular polymers (TEP). Since one may expect TEP to be glucose-rich, it is surprising that glucose uptake was not apparently affected.

But I wonder if there are some effects of pCO2 on at least the P properties. I see differences in the peak heights in Fig 1 and certainly differences in APA activity on days 13-19 (Fig 3). Perhaps there are differences in Fig 2B, but I find these types of graphs difficult to interpret.

The authors point out that because they analyzed samples from only one of the three mesocosms they did, the power of their statistical tests is limited. Perhaps others would object and call this pseudo-replication, but it seems that the authors can use analytical errors and the differences over time to make conclusions about pCO2 effects. I suspect that the analytical errors and the temporal variation, which the authors have measured, are larger than the variation among mesocosms.

Specific comments

1. page 3938, line 20: I dont think general conclusions about DOC can be made from the authors´ results and certainly don´t think this statement about the lack of DOC limitation can be made in the Abstract.

2. page 3940, line 10 and later: The authors define specific affinit as glucose uptake divided by biomass. But the definition really should be uptake at low concentrations, much below Vmax.

3. page 3942, line 4: What was the concentration of PO4 added in these uptake experiments? What was the incubation time?

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4. page 3942, line 19: What was the concentrations of added 14C-glucose?

4a. I suspect the added 14C glucose concentration was much higher than ambient concentration. Thus, the measured uptake was close to Vmax. This has to be mentioned and discussed, even if briefly, in the Results and Discussion section.

5. page 3942, line 24: The authors measured respiration of 14C-glucose, but didn´t report any results. I think they should, at least the averages. There arent too many estimates of % respiration for marine waters.

6. page 3944, section 2.4: I don´t see the need for this section. The word definitions of specific affinity for phosphate and glucose are adequate.

7. page 3944, line 4: Rather than ´unify the unit´, the authors ´made the units consistent´ or more simply, ´the same.´ That is, they should re-word this to avoid having unify and unit in the same sentence.

7a. But more importantly, the authors really never compare glucose and P uptake and so there was no need to unify the units. I suspect all of their conclusions could be made with units of P or glucose uptake per cell, thus avoiding assumptions of P and C content per cell.

8. page 3947 bottom and top of page 3948: The authors have a complicated summary of the different phases of these mesocosms. I wonder if a table summarizing these phases and data would be useful and clarify this discussion.

9. page 3948, line 24: The comparison of glucose uptake and DOC concentrations is a correlation problem, not regression: both have errors, neither was experimentally controlled, and it´s not clear which is the independent variable. So, rather than r^2, r should be reported. The correlation coefficient will be a higher, negative number, both properties helping the authors´ cause.

10. page 3950, line 12: I like the comparison of the measured glucose uptake with that expected in a diffusion limited situation, but the data do not support any conclu-

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sion about DOC limitation. It is very difficult, if not impossible, to go from the uptake properties of a single compound to the uptake of a complex pool of many compounds (some wildly different from glucose) that is DOC. Suppose that glucose was naturally very low in these mesocoms, such that bacteria did not express high affinity uptake systems; why make an expensive transport system if there is nothing to take up? In a short incubation, the bacteria would not have enough time to upregulate a high affinity system and thus the measured properties of the existing system would be much lower than expected.

11. Figure 6B: What is driving the negative relationship between glucose affinity and DOC? Glucose uptake or bacterial biomass, both of which contributes to the calculated glucose affinity?

Interactive comment on Biogeosciences Discuss., 4, 3937, 2007.